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Biogeosciences
Discussions

Interactive comment on "Modeling the impact of agricultural land use and management on US carbon budgets" *by* B. A. Drewniak et al.

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We thank the reviewer for providing helpful comments to improve our manuscript. We have expanded our analysis to include the suggested revisions wherever possible, and when suggestions were not possible to include in current study due to the model capabilities, we have included additional text to address those issues in the discussion. Our point-by-point response to the review follows.

Overall comments: This discussion paper investigated potential of the effect of crop residue removal on changes in soil organic carbon (SOC) stock in land under agricultural usages, at a large geographical scale. Simulation experiment using CLM-Crop model was applied for this purpose, in combination with spatial and temporal inventories on historical climatic data, land-use/land-cover, and crop-calendar. By using the

design of the simulation experiment very similar to that employed in previous study by Drewniak et al. (2013), this study focused on the changes in SOC stock. In addition, validation for the performance of the CLM-Crop model prediction for SOC stock over contiguous USA was conducted using gridded SOC stock data from IGBP-DIS and field observed SOC stock data from ISCN, respectively. Challenges to improve earth system models to deal with agricultural ecosystems, and especially, SOC stock changes, are of interest for a wide range of scientific community. Especially, the attempt to conduct model validation using field observed SOC data would attract a great deal of interest. My overall impression regarding to this manuscript is as follows.

Comment: 1. First of all, some of the authors' interpretations on the result of validation on model performance to predict amount of SOC stock on lands under agricultural use (i.e. model predicted SOC stock vs. observed SOC stocks in agricultural lands in ISCN data; Fig. 3) are rather questionable. Although, the authors postulated that CLM-Crop can capture the SOC stock at various agricultural sites, however, I think it is difficult to conclude that such statement is supported by the results presented in this study. Rather, it should be interpreted that model simulation failed to predict variations in SOC stock at various agricultural fields.

Response: The nature of global scale land surface models, such as the community land model (CLM) investigated here, is to represent large grid areas and cover different types of soil and landuse. The grid size is primarily driven by the need to simulate over hundreds of years and often coupled to an atmospheric model as a part of an earth system model. As these models are developed and improvements made to different components, we expect the resolution to increase and so would the ability to match data collected on a small 'sites' and at different depths. The primary focus of this manuscript and the model development is to represent in the model the basic features of the crops, their impact on carbon stocks above and below the soil at a model resolution. Given that, we have modified the text to reflect that the model results captured a wide range of SOC stock values of U.S. croplands, and stated that the model results showed substantial differences with individual field observations (P. 13684 L. 15-17). It would be difficult for any model formulated to be part of a global earth system modeling to capture the exact SOC value measured at any given location. Our intention was not to imply that the model could predict field observations perfectly, but it can capture a wide range of observed SOC values, making it a useful tool to evaluate the effects of management change on SOC. We argue that this is sufficient for the task at hand i.e. calculate the changes due to agricultural management in SOC, not the absolute values of SOC's by themselves. We have also included additional analysis looking at the SOC stock correlation to temperature and precipitation (see our response to "suggestions for revision" – 4 below). Despite the lower SOC estimate by CLM, the model does capture the trends in SOC across temperature and precipitation zones (i.e. decreases in SOC stock over increasing temperature and precipitation). Further, we also noted in the discussion (P. 13688 L. 6) that the differences we found between observed and modeled SOC stocks, suggest improvements are needed in the model structure such that they can better capture the cropland SOC dynamics.

Comment: 2. In addition, methodology for model validation using the ISCN data was too simple and not appropriate for the purpose and the question addressed. From the experimental setup it is obvious that there is a large gap in the size of spatial entity between model simulation prediction (a 2.8 $\times$2.8 grid, with varying area of soil columns) and observed soil data set (a field). Therefore, I think authors should build more elaborate strategies to compare observed SOC data with model output by, for example, filling this gap, before concluding just that large variations in field observations made the model validation difficult. The same can be said to the method of comparison for SOC stock between model predictions and observations in ISCN applied in this study, which just simply compared averaged SOC stock of soil columns in a grid, with differing depth (e.g. 0-300 cm or deeper for model simulation; 0-15 or 0-30 cm for most of observations in ISCN; according to the body text), without any attempt to minimize the effect of this difference on the comparison of SOC stock of soil columns by, for example, using uniform depth of soil columns for SOC stock calculation for both model

simulation and observations.

Response: We are aware about the mismatch between point observations and a CLM grid cell. Since the spatially interpolated observation data to the model grid scale do not exist for the US croplands and taking into consideration the reviewers concerns, we have modified our validation approach and replaced Figure 3. We instead plotted the mean observed SOC for each model grid cell scale along with its standard deviation (see our response to "suggestions for revision - 1" below). Regarding soil depth, all of the ISCN data was for depths of 1 m; we have clarified this distinction in the paper (P. 13683 L. 24 and P. 13684 L. 25). Although CLM calculates SOC in a 3.8 m soil column, since most SOC is within the top meter (Jobbagy and Jackson, 2000) we feel that the depth mismatch between the simulations and observations will be minimal. As per the reviewer's suggestion, in this approach we didn't remove any observations.

Comment: 3. Although, authors postulated that CLM-Crop model could capture the range of SOC stocks observed in agricultural fields (Fig. 2), however, it is difficult for me to judge whether this was true because of above mentioned reason. I also have questions with regard to 1) reasons why observed data records in ISCN having SOC value greater than 50 kg C m-2 were excluded (Fig. 2 and Fig. 3), and 2) potential bias in the ISCN dataset, if any measures like stratified random sampling had not been employed in sampling sites setup (please see specific comment).

Response: As stated previously, we used SOC observations to 1-m depth, which captures the bulk of SOC in the soil column. In addition, half of the observed SOC values were at or below 10 kg C/m2 and 75% were at or below 15 kg C/m2, which is the range of the modeled output. Given this distribution, despite the underestimation of SOC in CLM, the model did capture a wide range of SOC in US croplands. Of the over 4000 cropland data points from the ISCN that were included in the analysis, only a small portion (2.5%) had values greater than 50 kg C/m2. Most of those observations did not fall within a CLM grid cell growing crops. With the new approach of gridding the data for Figure 3, we kept those points in the analysis (which are reflected in some of the large standard deviation calculations). We still excluded those points in Figure 2 only (to improve readability of the figure) since they are all outliers and cropland SOC values are typically less than 50 kg C m-2 (Mishra et al., 2010, 9.5 kg m-2; Kern 1994). Most of the ISCN SOC data of continental US comes from the USDA-NRCS database. In this study, we included all the pedons that were from the croplands. A separate study is ongoing that is segregating the dataset into different land cover types, climate regions, major land resource area, etc., but this will not be complete for some time. In fact, studies such as this point to the need for more aggressively developing these datasets as more and more climate models represent croplands as one of the landuse types in their models. We do not expect our model would capture actual site SOC observations since our model cannot capture changes in land use or management practices over time.

Comment: 4. I believe that the above mentioned points (1-3) are crucial, as many of subsequent discussions on the size of the effect of residue removal on SOC stock change were based on the advocated good performance of the CLM-Crop model to predict SOC stock in land under agriculture. Therefore, I rate this point as a major flaw.

Response: As discussed above, the type of models discussed here are a new breed of crop model implementations that would work within a coupled earth system model and provide the ability to distinguish a crop landuse type in these models. These models are currently in the process of being benchmarked with available datasets and developing new strategies for both improving the models and identifying data needs. We do agree with the reviewer that there is a need for higher resolution models that can be of a scale that will resolve individual croplands (several hundred meter resolution) and availability of data at these scales. Both of these are not possible at this moment (computational limitation on very high resolution climate models and unavailability of chronosequenced SOC data at very high resolutions). As better ways of representing available data becomes available and model resolutions improve we will be in a position to evaluate absolute values of SOC in the models. However, as long as we focus

on perturbations to SOC due to various disturbances, a reasonable representation of SOC over large grid cells and over long time scales as was done here is a reasonable approach. We addressed the reviewer concerns within these constraints and included additional analysis with new and existing observational sets (for details see our response to “suggestions for improvement” below). We believe the analysis strengthens the manuscript and thank the reviewer for the suggestions.

Comment: 5. About SOC stock of all land, model predicted SOC stock over all land-use types over USA, 84 Pg C, was found to be comparable with previous estimation by Kern (78-85 Pg C; Kern, 1994). However, more detail explanation on the setup of model input data for historical land-use change is needed to interpret meaning of this result (please see specific comment).

Response: The estimates of SOC stocks by Kern (1994) were based on data from several sources, between 1975-1990, whereas in our model land use data comes from Leff et al. (2004), which is representative of the early 1990’s. We agree that this discrepancy will cause changes in model predictions, however CLM cannot support dynamic land use with functioning crops (this includes changes in cropland extent and crop type). As a result, we were only able to introduce land use change once after the model spinup. However, our simulations without cropland (i.e. crops represented as grass) indicate about 93 Pg C stored in US soils. Therefore the full range of SOC values between no cropland and the current cropland distribution could be between 93-84 Pg C, with the actual value somewhere in between. Since the bulk of SOC loss (20-50%) occurs in the first 40-50 years (Lal et al., 1999), we don’t anticipate the SOC loss by the model to be exaggerated, which is why our prediction still falls within range of the estimates. As suggested by the reviewer, we have included additional information in Section 2 of the manuscript to clarify how historical land use was simulated by CLM (P. 13682, 1st paragraph). We have also expanded our discussion to include uncertainties in the CLM estimate of SOC from historical changes in land use, intensity of agriculture, and fertilizer usage (P. 13688 L. 10).

Comment: 6. In conclusion, I rate the paper is not acceptable in present form, and recommend that the paper should be revised so that it will be evaluated again whether or not it can be accepted for publication in BG. I recommend authors to revise the paper with taking into account the above mentioned points as well as specific comments shown below. I would like to encourage the revision, as a challenge to conduct validation of model performance to predict SOC stock change using field observed data is important and would attract a great deal of interest for scientific community. I included some suggestions for revision.

Response: We have performed additional model validation per the reviewer's suggestions, which we believe strengthened the manuscript. Our findings indicate CLM can capture the change in SOC after conversion to cropland over long time periods, although the model does not simulate the initial rapid loss of SOC. Our additional analysis showed that CLM captures climate trends in SOC stocks; particularly, simulated SOC decreases with increasing temperature and precipitation as supported by observations. Finally, we extended the discussion section of the manuscript to address reviewer's suggestions that current CLM model structure does not permit and thus we were unable to accommodate (e.g. changes in land use) (P. 13688 L. 10).

Specific comments: Land-use change:

Comment: Please add more detail explanation on historical land-use change setup for the simulation. From the body text, it seems that land-use change (i.e. conversion of grasslands to croplands) was set to occur only once throughout the entire time sequence of simulation, and all at once at the end of spin-up (in 1850; if I understood correctly). However, land-use/land-cover data used to assign land-use conversion from grassland to cropland corresponds to early 1990s; e.g. land-use/land-cover dataset used for grass scenario (IGBP DISCover) corresponds to 1992-1993 (Loveland et al., 2000), whereas that for other scenarios represents the early 1990s (Leff et al., 2004). Therefore, I wonder whether the occurrence and duration of cropland land-use was largely overestimated in the simulation. If this is the case, I do recommend revising

the input data of historical land-use change to include several land-use change events to be more consistent with the changes in cropland area estimated by Ramankutty and Foley (1999) and to re-execute model simulation with the revised input (I am not sure whether the model can deal with land-use change to occur several times during the course of simulation, though). As the simulated SOC loss in a grid was found to correlate with area of agricultural lands in a grid, this point may be crucial.

Response: We've added text to clarify how the land use conversion in CLM occurred (once during the simulation since CLM cannot support changes in land use at this time; P. 13682, 1st paragraph). The model spinup is designed to get the model in a steady state only and does not correspond to an end period year of 1850. The 171-year simulation is chosen to simulate the most intense agricultural land use over the U.S. We agree that using a modern land use map (representing early 1990's) over the entire period may overestimate the agriculture land area, but historical cropland land use peaked in 1940, followed by a decline (the strength varies with region; Waisanen and Bliss, 2002). Furthermore, agriculture land use was established prior to 1850 so we expect that the model may overestimate SOC in some grid cells where cultivation has been practiced longer. As such, we feel confident that our choice in land use is amenable given the model constraints. However, since the effect of land use change is important for SOC, we have modified our manuscript to include a discussion on historical land use change impacts on SOC (P. 13688 L. 10).

Comment: Organic matter input to soils: Input of manure from live-stock waste origin to soils was not taken into consideration in the model simulation. According to MacDonald et al. (2009), about 15.8 million acres of cropland, equivalent to about 5 percent of all U.S. cropland in 2006, were estimated to receive livestock manure. Although, this figure is just an estimate and showing that manure is used on only a small fraction of U.S. cropland, however, for some major crops the percent of the acreage received manure may not be negligible, such as corn (12 %), oats (9 %), as well as hay and grasses (7 %) (MacDonald et al., 2009). Although, input of manure, and in addition, sewage sludge, is taken into account in the estimation of SOC stock change in greenhouse gas inventory reporting of USA, I wonder whether these contribution can be considered as negligible or not. I also think that the title of the manuscript employing the term, 'US carbon budgets' is rather exaggerated.

Response: We thank the reviewer for suggesting alternate source of nutrient addition to the croplands. However, in current model structure of CLM there is no mechanism for adding organic matter to soils through livestock manure although this is a focus for future model development. Therefore, we have considered this suggestion as a source of possible uncertainty in CLM results, and expanded the discussion section to address this issue (P. 13688 L. 10).

Comment: Soil organic carbon stock: P. 13683, L.6-8: In "The total stored SOC over all land surface types in the United States, as calculated by CLM-Crop, is 84 Pg C, which falls within the range of previous estimates of 78–85 Pg C (Kern, 1994).", please explain for which year the prediction and the estimate was made, respectively. From Fig. 4, it seems around 2020 is refereed (i.e. 'Current Residue' in 2020 (i.e. 1850 + 170 = 2020) at around 85 Pg C) for the model prediction. If this is the case, I wonder if this corresponds to the year for which the land-use/land-cover estimation by Kern (Kern, 1994) was made.

Response: See also our response under "Land-use change". We have removed the reference to 1850 in the manuscript since our intention was not to indicate that our simulation was starting at 1850, but to reference the intense period of agriculture production in the U.S. Our rational for running the model for 171 years was to compare the transient model state after heavy agriculture establishment with observations. Therefore, we have added clarification that our simulation doesn't correspond directly to the years 1850-2020. We think the Kern (1994) dataset is roughly representative of the 1990's, as it contains the pedons data collected by USDA-NRCS through a long period of time (1975 – 1990). Since we are aware that the uncertainty with this approach is substantial, we have included additional analysis (Section 3.2) that examines the

change in SOC after cropland establishment to validate our model results.

Comment: P.13683, L.28: What is the reason to exclude the plots with SOC > 50 kg C m-2? Is this because ISCN data has problems in data quality control? Any organic soils included? Please explain.

Response: Of the over 4000 data points from the ISCN that were included in our analysis, only a small portion (105 points, 2.5%) had values greater than 50 kg C/m2. Since cropland SOC values are typically less than 50 kg C /m2 (Kern 1994; Mishra et al., 2010, 9.5 Kg m-2) we initially removed the higher values from our analysis. In addition, the land use designation in the ISCN data may not reflect the land use when the soil measurement was taken, which could result in erroneous data points collected for our analysis. Our modified analysis approach taking the mean SOC stock with SD now includes all observation data points (with the exception of Figure 2 to improve readability of the graph). We've added text to clarify (P. 13683 L. 28).

Comment: Fig. 2 and Fig. 3: I wonder whether the soil sampling site selection in ISCN employed stratified random sampling, with taking different land-use, management, soil types, and climate regimes into account, or not. If not, as is often the case with many of existing soil data set, the data should be dealt in a careful manner especially when the entire data is just simply compared with model output because of potential bias.

Response: Most of the cropland SOC data in ISCN database comes the pedon data collected by USDA-NRCS. Stratified random sampling was not employed while collecting these samples but the idea was to represent different soil types using expert judgment of pedologists. As suggested by the reviewer, in additional analysis, we have analyzed the distribution of SOC samples across temperature and precipitation gradients (see our response under "Suggestions for revision – 4").

Suggestions for revision:

Comment: 1. Employing calculation for weighted means for SOC stock for each grid based on observed SOC data, of ISCN or including additional soil dataset, with taking into account relative distribution of different land-use type and history, soil types, management practices, etc., in each grid, if possible. Number of data may not be enough to conduct such calculation, though. It would be useful to consult methodologies used for the estimation of SOC stock change at country scale in the national greenhouse gas inventory reporting of USA (USEPA, 2014), which employs an 'expansion factor' for scaling of SOC stock (stock-change) from observation points to the entire country.

Response: We agree that weighting the SOC stock from observations to aggregate to the grid scale is important for the comparison between observations and the model output; however, conducting this study is outside the scope of present study. We have reconsidered our analysis and replaced Figure 3. Rather than plotting all the observational points compared to the model, we averaged the observation data within each model grid cell and included the standard deviation of the observations in each grid cell. This demonstrates the large variability of SOC observations within each gridcell. With this additional analysis, we are also able to infer that the model tends to estimate higher SOC in sandy soils, and lower SOC for clay and silt soils, although it should be noted that the soil texture is determined from the model since observational data does not include soil texture. We include this additional analysis in Section 3.2

Comment: 2. Referring to figures of estimated SOC stock (stock-change) shown in the national greenhouse gas inventory reporting (US-EPA, 2014) and to compare it with that predicted in the model simulation in this study. Such information, and, in addition, corresponding discussions will be useful for readers.

Response: We thank the reviewer for pointing out the US-EPA (2014) report, but note that the carbon fluxes estimated were also model-derived using the DSSAT model. Therefore, we found additional observational studies to compare our change in soil carbon from Wei et al. (2014). The dataset includes 453 site observations from 36 countries of SOC change when forests are converted to cropland. SOC stock change was reported for time periods ranging from 1-200 years from conversion. We include

an additional figure in the manuscript (Figure 5), which shows the percent SOC loss over a 200-year period as reported in Wei et al. (2014) along with a random sample of 500 points from CLM. The data indicate that although CLM cannot simulate the initial rapid loss of SOC following land conversion, over long periods CLM does capture total SOC loss. Since decreases in SOC increase with increasing initial SOC concentration (Wei et al., 2014), we believe the initial modest decline in SOC is the result of lower initial SOC concentrations simulated by CLM. We have included this figure and accompanying discussion in section 3.2 of the manuscript.

Comment: 3. Use of information on land-use/land-cover, land-use history, and management practices of each sampling site in the ISCN data set when assessing the range, mean, or median of SOC stocks, if such data are available. Again, uniform depth of soil columns should be used in the calculation to compare it with model simulation output. As this study emphasized that differences in crop residue input to soils would have affected changes in SOC stock significantly, such information on land-use (and management, if available) should be included in the method for model validation.

Response: We agree that segregating the data in the suggested manner would be informative, but is beyond the scope of this study. An effort to evaluate the data is ongoing and these results can be re-evaluated at that time. Although our soil depth in the model (3.8 m) is deeper than the observations (1 m), we feel that the mismatch has little impact on results since most SOC is in the top soil layers (Jobbagy and Jackson, 2000). Most field data is not collected beyond 1-m and the current CLM model structure doesn't have a vertical carbon profile. We addressed the issue of changing land use and management further in the discussion section of the manuscript (P. 13688 L. 10).

Comment: 4. In addition, assessing differences in SOC stock between different land-use types (e.g. cropland vs. grassland) at several different geographical zones having different climatic conditions (temperature, precipitation etc.), in model and observation, respectively, followed by making comparison between these two to explore model performance. I expect such approach would produce output that may highlight strength of the study applying earth system model and spatial and temporal inventories of climate and land-use over large geographical entity.

Response: We thank the reviewer for this suggestion and have performed additional analysis to consider the impact of climatic conditions (temperature and precipitation) on SOC stock. Though the CLM simulates lower SOC stocks initially, the trend in SOC stocks over the different climate regimes mirrors that of the observations. High temperature and precipitation regions result in higher turnover rates of carbon (Wei et al., 2014) and result in lower SOC stocks. Over natural vegetation, the variability of SOC is quite high, but CLM hints that higher precipitation results in higher SOC. This could be the result of higher vegetative productivity when sufficient rainfall is present. A figure plotting the SOC stocks from model simulations and from data observations against annual mean temperature and total precipitation (Fig. 4) and accompanying discussion has been added to the manuscript in section 3.2.

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